

LETTERS TO THE EDITOR.

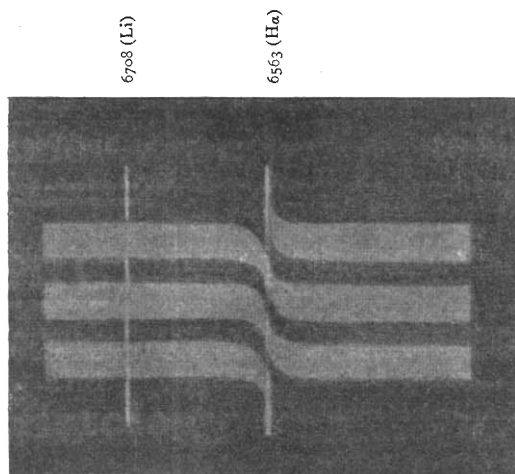
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Anomalous Dispersion of Luminous Hydrogen.

ON pp. 413 and 607 of vol. lxxvii., and p. 55 of vol. lxxviii., of NATURE, Prof. Schott and Mr. Norman Campbell discuss the question of "The Theory of Dispersion and Spectrum Series." Though not desirous of reopening this discussion, we think the readers of NATURE may take some interest in the results of experiments we have just finished upon the anomalous dispersion of luminous hydrogen.

We used the continuous spectrum given by a narrow capillary tube when filled with hydrogen at nearly atmospheric pressure, and traversed by a convenient current given by a large induction coil. In that spectrum we generated horizontal interference fringes by using a Jamin interferential refractor (cf. L. Puccianti, *Nuovo Cim.*, ii., p. 257, 1901), and we sent one of the two rays between the Jamin mirrors through a Geissler tube filled with hydrogen of about 4 mm. pressure.

When this tube is put in series with the capillary tube above mentioned, the interference fringes at both sides close to the red hydrogen line (H_α) suddenly change their direction, as in the accompanying figure, showing directly the



anomalous course of the refractive index near the "absorption line." By measuring the maximum variation of the refractive index (8×10^{-7}) and the breadth of the H_α line (2-3 Å) we find, according to the Drude-Voigt theory of dispersion (cf. W. Voigt, "Magneto u. Electro-optik," p. 114, 1908), that the ratio of the number of "electrons of dispersion" to that of molecules of hydrogen is only about 1 to 50,000, and that the damping-constant (cf. Voigt), measured in wave-lengths, is of the order 2-3 Ångström units.

We have not succeeded in detecting anomalous dispersion at the other hydrogen lines, which is expected to be much smaller than that at the H_α line, on account of the smaller absorption (cf. R. Ladenburg, *Verh. d. deutschen phys. Ges.*, x., p. 550, 1908).

We conclude that our experiments show that it is not possible to explain the dispersion of luminous hydrogen by the existence of one class of electrons only as in the case of non-luminous hydrogen; we have to introduce new "electrons of dispersion," and the frequencies of these seem to be those of the lines of the so-called first series of hydrogen.

RUDOLF LADENBURG.
STANISLAW LORIA.

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October 17.

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The 4.79 Period of Sun-spot Activity.

IN NATURE of August 13 (p. 351) the photograph is published of two groups of sun-spots taken on August 6, and attention is directed to the remarkable fact that such an outbreak should occur two years after the sun-spot maximum. This renewed sun-spot activity is connected with the 4.79 period, which I have shown to have been quite persistent—even more so than the eleven-year period—since sun-spots were first systematically observed. In a paper published in the Philosophical Transactions for 1906 I gave the times of maxima of this period as being $1903.72 + 4.79 n$. This would bring the maximum to 1908.51, or to about July 1 of the present year. A retardation of one month in a period of more than four years' duration is, of course, insignificant.

Simla, October 19.

ARTHUR SCHUSTER.

Memory in the Germ Plasm.

DR. ARCHDALL REID repeats (NATURE, October 15, p. 605) his well-known opinion that from infancy forwards a man develops physically and mentally, principally under the stimulus of use, and he adds, "*the muscles of an infant's limbs do not grow unless used*. His mind is almost blank at birth, but grows under the influence of experience (use). In this way he learns to coordinate his muscles, and a vast deal more."

There are no italics in the original, but it is to these two statements that I desire to direct attention.

In regard to the first of them, we may well ask what evidence Dr. Reid can adduce for such a statement. It would be nothing but a vague and even false analogy if he relies upon what happens when limbs are paralysed owing to damage to the spinal cord. Physiologists generally would surely believe that the muscles of an infant tend to grow after birth, just as its bones tend to grow—those of the skull, for instance—quite irrespective of use, however much the process may in some cases be accelerated by use.

Then, again, there are crucial facts to show that in regard to many most complicated movements it is not necessary for a child to "learn to coordinate his muscles." On the contrary, the possibility of coordinating very many muscles, even for such very complex acts as speaking and walking, is brought about as a result of the inheritance of cell and fibre groupings in the brain and spinal cord which only become perfected after birth. It is true that for both these complex muscular acts it commonly happens that trials and failures are made while the nerve mechanisms are developing—hence children may seem to acquire these accomplishments solely as a result of experience. But the real all-important share of inheritance in bringing about the possibility of performing the complex muscular acts in question is conclusively shown by cases in which, from some cause, speech or the ability to walk is delayed to a comparatively late period—when the related nervous mechanisms have had time to become developed. Then, under the influence of some strong excitement, a child who has never spoken a word up to two or even five years (but whose sense of hearing is good) may suddenly begin to speak clearly without antecedent attempts of any kind. Cases of this sort may be found recorded in my work "Aphasia and other Speech Defects" (pp. 5-8).

The same kind of thing may occur in regard to walking. When mentioning the cases of untaught speech above referred to one day to the late Sir Richard Quain, he told me that one of his children, up to the age of two years, "had not walked a step, or even tried to walk, when one day he put her down in the standing position, and to his great surprise, as well as to that of the nurse, she walked from one side of the room to the other." This also was an untaught act, as there had been no previous trials and failures ("Brain as an Organ of Mind," p. 607).

Thus, because insects and many other animals, as Dr. Reid says, "come into the world fully equipped physically and mentally to cope with their environment," and man does not, it does not at all follow that the inherited formative tendencies of man may not go on to a considerable extent after birth, even though use, in the majority of cases, does come in as a cooperating cause

while the necessary nerve mechanisms are developing. To ascribe so much to use, as Dr. Reid does, and so little to inheritance, is surely a grave error. Speech has certainly been acquired by the human race, and it is an accomplishment which is not learned afresh by each one of us as he would have us believe—we inherit the nervous mechanisms that make it possible, and these tend to develop even independently of use. II. CHARLTON BASTIAN.

The Athenæum, October 20.

I FEAR I must think that the crucial instances which Dr. Bastian mentions are merely cases in which the observer, having a preconceived theory, has preferred an improbable interpretation to an obvious and simple one. The muscles of the limbs atrophy when disused through joint disease as well as when the injury is in the spinal cord. At the same time the nails, which do not develop under the stimulus of use, continue to grow. It is always difficult to prove the excessively obvious in a few words; and to me—if anyone ever learns anything—children as obviously learn to walk and speak as to write and swim. Dr. Bastian would have us believe that people who have never heard a word would still be able to express their thoughts in language. But in what language? How does it happen that children always speak the language of the people with whom they are reared? My parents were English. My first language was Hindustani. Which of the two was innate? Structures (e.g. external ears), which do not develop under the stimulus of use, do not atrophy through disuse. So also instincts never atrophy—are never forgotten—through disuse. How does it happen that I have forgotten my first language?

G. ARCHDALI REID.

Netherby, Victoria Road, S., Southsea, October 27.

Polypus Vinegar—Sea-blubber Arrack.

(1) ALTHOUGH I am afraid it is now much too late to reply to Mrs. Hoskyns-Abrahall's inquiry anent the so-called *Polype vinaigre* (NATURE, August 9, 1906, vol. lxxiv., p. 351), to which hitherto no answer has appeared in your columns, I may be allowed to quote the following passage as a probably important clue to its scientific elucidation:—

"Amongst the greatest curiosities of the Yellow Sea there is a wonderful polypus, only recently discovered. This curious zoophyte is known on the coast of Newchwang by the name of *Chang-yu*, and possesses the property of turning into vinegar the fresh water in which it is placed. This fact was noticed for the first time in Huc's travels in China and Thibet, but our savants at home were rather sceptical on the point, and refused to believe in its existence till it was lately sent to Paris by another missionary, Mr. Pernys, and the specimens, one alive and one dead, being put in tank at the aquarium of the Société d'Acclimatation, they both turned into vinegar the fresh water in which they were placed" (A. Fauvil, "The Province of Shantung," in the *China Review*, vol. ii., No. 6, 1875, pp. 366-7).

So far as my limited reading goes, not a single Chinese work mentions or describes this remarkable creature. But I may hazard a remark that peradventure by *polype* Huc really meant a cephalopod, for the "Pen-tsao" applies the name *Chang-yü* (not *yu*) to the octopus, which formed a member of the classic authors' Polypi, as is manifest in Pliny's "Natural History," bk. ix., ch. 48 (see also the "Encyc. Brit.," ninth edition, vol. xix., p. 428).

(2) In "A New Account of East India and Persia in Eight Letters, being Nine Years' Travels, begun 1672 and finished 1681," by Dr. John Fryer, F.R.S., published London, 1698, pp. 68-9, the writer, recounting the causes of the bad health of the inhabitants of Bombaim, an island situated sixty leagues south of Surat, and the same distance north of Goa, says, "Among the worst of these, Fool Rack (Brandy made of *Blubber*, or *Carvil*, by the Portugals, because it swims always in a blubber, as if there were nothing in it; but touch it, and it stings like nettles; the latter, because sailing on the Waves it bears up like a Portugal Carvil; it is, being taken, a Gelly, and dis-

tilled causes that take it to be Fools), and Foul Women may be reckoned."

It is well known that certain species of jelly-fishes are eaten with gusto by the Japanese and the Chinese, but we have never heard, except the above instance, of any aculeph capable of yielding a spirituous liquor. Will any of your readers kindly tell whether it is fiction or truth?

KUMAGUSU MINAKATA.

Tanabe, Kii, Japan, August 6.

Occurrence of a Fresh-water Nemertine in Ireland.

IN NATURE, 1902 (vol. xli., p. 611), Prof. Benham records the discovery of a fresh-water Nemertine living in the River Cherwell, at Oxford. He found only a single immature specimen, which was accidentally destroyed before the specific title was definitely determined. I have recently (October) obtained numerous sexually mature specimens of a Nemertine, living among weeds in the Grand Canal, at Clondalkin, co. Dublin.

Seven species of fresh-water Nemertines, all belonging to the genus *Prostoma* (*Tetrastemma*), are recognised by Bürger (*Tierreich*, vol. xx., p. 68). The distinctive characters are somewhat vague, and depend largely on differences in the mode of reproduction. The Irish forms are referable to the species *Prostoma clepsinoides*, Ant. Dugès, with which the *Tetrastemma aquarum dulcium* of Silliman is probably synonymous. Benham notes several points in which his specimen differed from the latter species, and the Irish specimens show the same differences. Such points, however, as the relative position of the eye-spots and ciliated pits depend largely on the state of contraction of the worm, or it is possible that Silliman made his drawings from pressure preparations. As Benham points out, the proboscis is much longer than Silliman shows. Benham also says that the colour of his specimen was due to pigment in the skin, and not to the red colour of the nervous system. In the Irish worms, the epidermis is only faintly yellow in colour, whilst the brain and nerve cords are bright red, as is usual in the Nemertines.

This species was also found by Beddard ("Cambridge Natural History," vol. ii., p. 118) in one of the tanks in the Botanical Gardens, Regent's Park.

These are the only records of fresh-water Nemertines in the British Isles, and it is highly probable that they refer to the same species, for which the name *Prostoma clepsinoides*, Ant. Dugès, has priority.

ROWLAND SOUTHERN.

Natural History Department, National Museum,
Dublin, October 22.

Mercury Bubbles.

I SHOULD be glad to learn through the medium of your columns if any previous attempt has been made to produce mercury bubbles, and, if the attempt was successful, where was the result described? A few days ago, while in the act of purifying mercury by the common method of treatment with acid, and afterwards washing with a powerful stream of water, I was surprised to notice quite frequently several beautiful silvery spheres circulating on the surface of the wash-water. As to dimensions, many of these spheres were at least 22 mm. in diameter, and I estimated the thickness of the metallic film in one case to be 0.017 mm.

The bubbles seemed to be produced by the jet of water entangling air at the moment of striking the surface of the water in the containing vessel, and thus carrying the air into the body of the mercury, the rapid circulation of the wash-water helping to disengage the bubbles from the surface of the metal as they were formed. It is just possible that the air was not derived from the surrounding atmosphere, but was contained in the water supply. I should add that in my laboratory the pressure averages about 60 lb., and there is undoubtedly at times a relatively large amount of air present.

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